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One hundred and two presents were announced as having been received since the last meeting, including, among others :—

Galileo, Opere, Edizio Nazionale, vol. 8, presented by the Italian Government ; Leipzig, Astronomische Gesellschaft Catalog, Zone $-2°$ bis $+1°$ (Nicolajew), presented by the Society ; F. Nansen, Scientific Results of the Norwegian North Polar Expedition, pt. vi. Astronomical Observations, arranged and reduced under the supervision of H. Geelmuyden, presented by the Editor ; Potsdam, Astrophysikalisches Observatorium, Photographische Himmelskarte, Band 2, presented by the Observatory ; H. H. Turner, Modern Astronomy, presented by the Author.

Lantern slides from photographs of the total solar eclipse of 1900 May 28, presented by Count de la Baume Pluvinel.

On the Oxford Photographic Determinations of Stellar Parallax. Reply to the Criticisms of Sir David Gill. By H. H. Turner, D.S.C., F.R.S., Savilian Professor.

In the *Annals of the Royal Observatory, Cape of Good Hope*, vol. viii. Part II., "Researches on Stellar Parallax made with the Cape Heliometer," recently published, Sir David Gill criticises adversely the observations of stellar parallax at the Oxford University Observatory in the years 1886 to 1892, saying that they "cannot be regarded as of proved value, and are certainly not reliable within limits corresponding to the probable errors assigned to them by Professor Pritchard" (Introduction, p. xv). I had nothing whatever to do with this work, which was completed and published before I succeeded Professor Pritchard as Director of the Observatory. But the reasons given by Sir David Gill for his opinion are based on the printed figures, which lead me to such diametrically opposite conclusions that I feel bound to draw attention to the matter. The main reason given for condemning the results is that the determination of scale value on the photographs is faulty. These are Sir David Gill's actual words :—

"Pritchard's method of determining the scale value would be justified if numbers of pairs of stars on the same plate were measured, and the correction for scale value was derived from the mean of all these measures instead of from only one pair. But this is precisely what Pritchard does *not* do. In his first publication on stellar parallax (Fasciculus III., *Oxford University Observations*) he measures two pairs of comparison stars for each investigation of parallax, but he derives corrections for scale value separately from the measures of the stars $a-b$ and $c-d$. It is evident that if the observations were reliable we should

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have within accidental error of observation the same correction for scale value per 1000″ from the observations of both pairs; but this is far from being the case. For example we find :—

Difference from Assumed Mean.

1886.	$a-b$.	$c-d$.
June 23	+0″·885	−0″·455
June 24	−0″·742	+1″·199

"These, it is true, are extreme cases, but *throughout the series there is little evidence of systematic agreement even in the signs of the scale-value corrections*, and their average discordance is very much greater than the probable accidental error of observation which has been derived by Professor Pritchard."

Now these figures and the statement which follows them are, in my opinion, both wrong. In the first place as regards the figures. The impression given, I think, is that they represent the correction for scale value per 1000″, whereas they are quoted for $a-b$ from Table I. on p. 6, which gives the correction in 2382″; and for $c-d$ from Table XIV. on p. 40, which gives the correction in 2066″. If Sir David Gill had read the work with more care he would have seen that Professor Pritchard has rendered the figures comparable by giving in Table XIII., on pp. 38, 39, the corrections per 1000″ for $a-b$ and $c-d$ side by side; and he would also have noticed that one of the four quantities quoted by him has, by a typographical error, a wrong sign, viz. the value of $c-d$ for June 24 is −1″·199 and not +1″·199. (He could also have inferred this from the column immediately preceding it on p. 40; three numbers following are given with wrong sign: they are, I think, the only errors of sign in the two tables.) Thus the real figures for the correction per 1000″ are as follows :—

Difference from Assumed Mean.

1886.	$a-b$.	$c-d$.
June 23	+0″·372	−0″·220
June 24	−0″·312	−0″·581

which give a very different impression from those quoted above.

It is particularly unfortunate that this Table XIII. escaped Sir David Gill's attention, for it is immediately followed by these words of Professor Pritchard :—

"The inspection of the table suggests, I think, that the principal cause of the variations in question lies not so much in accidental or local variations of the film as in actual variations in the focal length of the mirror. *This suggestion seems to me to be borne out by the general prevalence of the same sign being attached to the variations on the same night.*"

I have italicised sentences in the quotations from Sir David Gill and from Professor Pritchard in order to bring out the fact that they are directly at issue in their views of

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the same figures. And now which of them is right? The point is a most important one (at least for the value of the Oxford work), and I venture to repeat Table XIII. in a slightly modified form, arranging it in order of the mean value of the two corrections instead of in order of date as given; but the dates are added, so that anyone who has not a copy of the work can reconstruct Table XIII. in its original form if he so desires. To eliminate the effect of accidental error to some extent means of every ten numbers down the series have been taken. The year is omitted from the date for brevity's sake; for though the observations extended from 1886 May 30 to 1887 May 31, there is no risk of confusion. May 30 belongs to 1886, and all other dates in May to 1887.

Table XIII.

Rearranged from No. III. of the Astronomical Observations made at the University Observatory, Oxford.

Date.	Variation in Measure.		Date.	Variation in Measure.	
	a−b.	*c−d.*		*a−b.*	*c−d.*
	″	″		″	″
Jan. 5	−0·682	−0·592	Sept. 13	−0·246	−0·013
Jan. 12	−0·680	−0·542	Apr. 19	−0·274	+0·016
Jan. 20	−0·553	−0·509	Dec. 9	−0·199	−0·052
June 24	−0·312	−0·581	Nov. 23	−0·225	−0·025
Jan. 31	−0·492	−0·398	Sept. 11	−0·154	−0·080
Dec. 14	−0·476	−0·398	Nov. 5	−0·061	−0·148
Feb. 8	−0·371	−0·442	Apr. 25	−0·144	−0·047
Dec. 16	−0·570	−0·198	Nov. 16	−0·177	−0·004
May 13	−0·401	−0·336	June 28	−0·106	−0·046
Jan. 8	−0·424	−0·312	May 20	−0·198	+0·079
Mean ...	−0·496	−0·431	Mean ...	−0·178	−0·032
Feb. 17	−0·369	−0·302	Dec. 4	−0·170	+0·063
Dec. 24	−0·508	−0·136	Aug. 28	+0·095	−0·179
May 14	−0·372	−0·196	May 9	−0·038	−0·040
Mar. 16	−0·269	−0·256	Sept. 30	+0·102	−0·176
May 10	−0·202	−0·273	Aug. 20	−0·053	−0·019
Sept. 27	−0·183	−0·281	Dec. 2	−0·047	+0·003
May 5	−0·222	−0·227	Oct. 21	−0·046	+0·004
June 1	−0·175	−0·273	May 30	+0·135	−0·171
Sept. 18	−0·172	−0·262	Oct. 13	−0·017	−0·013
Sept. 15	−0·272	−0·161	June 15	−0·179	+0·155
Mean ...	−0·274	−0·237	Mean ...	−0·022	−0·037

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Date.	Variation in Measure.	
	a−b. ″	*c−d.* ″
June 16	−0·102	−0·315
Sept. 22	−0·122	−0·292
Sept. 17	−0·220	−0·179
May 16	−0·360	−0·031
May 18	−0·205	−0·186
Aug. 29	−0·167	−0·207
Feb. 26	−0·210	−0·150
Dec. 7	−0·196	−0·148
May 7	−0·134	−0·181
Nov. 18	−0·127	−0·155
Mean ...	−0·184	−0·184
Oct. 2	−0·003	+0·070
May 26	+0·011	+0·109
Dec. 1	+0·059	+0·063
Oct. 6	+0·029	+0·109
Mar. 12	+0·021	+0·118
June 23	+0·372	−0·220
Oct. 22	+0·094	+0·058
Feb. 25	+0·120	+0·066
Sept. 7	−0·045	+0·238
Jan. 10	+0·101	+0·092
Mean ...	+0·076	+0·070
Sept. 10	+0·200	+0·042
Jan. 25	+0·114	+0·136
May 31	+0·102	+0·150
Aug. 24	+0·191	+0·142

Date.	Variation in Measure.	
	a−b. ″	*c−d.* ″
Apr. 26	−0·048	+0·035
Nov. 17	+0·031	−0·035
June 30	+0·295	−0·296
Aug. 30	+0·041	−0·038
Nov. 29	+0·016	−0·002
Apr. 30	+0·004	+0·011
Mar. 23	−0·093	+0·123
Apr. 29	−0·011	+0·061
Nov. 3	+0·010	+0·044
June 4	−0·126	+0·180
Mean ...	+0·012	+0·008
Mar. 27	+0·184	+0·155
Aug. 31	+0·183	+0·169
Apr. 2	+0·205	+0·170
Sept. 29	+0·236	+0·157
Sept. 20	+0·240	+0·184
July 1	+0·145	+0·316
Mean ...	+0·180	+0·162
Sept. 16	+0·326	+0·160
Feb. 5	+0·212	+0·312
June 8	+0·406	+0·223
Apr. 20	+0·223	+0·449
Aug. 26	+0·291	+0·415
Apr. 16	+0·558	+0·156
Feb. 27	+0·436	+0·303
Mean ...	+0·350	+0·288

Collecting the means of groups, we have the following correspondences:—

a−b. ″	*c−d.* ″	Difference. ″
−0·496	−0·431	−0·065
−0·274	−0·237	−0·037
−0·184	−0·184	0·000
−0·178	−0·032	−0·146
−0·022	−0·037	+0·015
+0·012	+0·008	+0·004
+0·076	+0·070	+0·006
+0·180	+0·162	+0·018
+0·350	+0·288	+0·062

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The general agreement is extremely good, although there are traces of a small systematic difference. It seems extraordinary that Sir David Gill should say: "Throughout the series there is little evidence of systematic agreement, even in the *signs* of the scale-value corrections." The most obvious supposition is that he looked at the figures so hastily as to make some mistake in assigning corresponding nights to one another. But is a hasty glance of this kind sufficient evidence upon which to condemn a great piece of work? There are some discrepancies in the individual nights which I felt might be due to numerical mistakes in computation. I have accordingly examined the original figures (the books being still at the observatory), and have found several such errors; but they do not materially affect the general result, and it is unnecessary to give them here. The greater part of them refer to the early observations, from 1886 May 28 to July 1. The reductions of these are mixed up with much experimental work in the determination of scale values, and in copying for the press it has, for instance, not been noticed that for the period June 23 to July 1, when a different mirror was in use, a scale value had been used for $a-b$ slightly different from that for $c-d$. When this is put right the extreme cases quoted by Sir David Gill, as well as another on June 30 (better seen in Table XIII., Table XIV. being here of wrong sign, as above remarked), disappear. But I cannot find the observations of June 28, and generally these early observations might reasonably be excluded. From 1866 August 20 the reductions are all made consistently and continuously, though there are a few errors in copying for $c-d$ from May 10–20, the removal of which would improve the agreement on May 20, not altering the other days much.

But it is not my present purpose to undertake a systematic re-examination of the whole work—merely to point out that Sir David Gill's criticisms, which refer to the printed figures, are not supported by those figures themselves.

Having set up this imaginary fault in the observations, Sir David Gill proceeds to explain it by an equally imaginary cause:—

"The obvious explanation is that in the process of development distortion of the film takes place, and this distortion is different in different position angles. As no réseau was employed, no means exist for eliminating the systematic error so produced except the crude assumption which Pritchard apparently adopted, viz. that this contraction of the film is proportional to the distance along any straight line passing through the centre of the plate, but different along different straight lines passing through the same centre. No attempt is made to test the accuracy or otherwise of such an assumption."

We have heard before of this distortion of the film. The réseau was proposed in 1887 as a means of obviating it; and in 1888 Sir David Gill included in a large scheme for measurement of the plates an elaborate determination of this distortion on

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each. His observatory has now been at work for some years on the measurement of stellar photographs, and perhaps we may soon look to *him* for "an attempt to test the accuracy or otherwise of such an assumption." Till we have positive evidence of some kind I for one shall venture to question the existence of this bugbear; and one of my reasons for this temerity is precisely the examination of these measures of Pritchard (or rather of his assistants), made without any réseau and yielding such accurate results. The réseau is a great convenience in measuring, but we have possibly altogether overrated its importance for the purpose for which it was invented. Of course a photographic plate is not perfect: there are all sorts of errors of a small kind—the granulation is itself a source of error, and the curvature of the glass, and its flexure, and so on. But these are of the nature of *accidental* errors, to be eliminated by taking plenty of photographs (as Pritchard did—four on each night) and measuring them under different conditions, not of *systematic* errors to be elaborately investigated. Nor did Pritchard necessarily use the different scale values in different directions because of possible distortion of the film. He did not believe in this distortion, as appears from the paragraph quoted from his work, and as I could show further by quoting from long letters to Sir David Gill, of which copies are in this observatory. But there may be very good reasons for using different scale values in different directions, quite independent of any hypothesis about distortion of the film. I mention one or two, quite on my own responsibility, not arising out of anything I have seen in Pritchard's work.

1. If the mirror or lens with which the photographs are taken is not an accurate surface of revolution (either essentially or because of flexure), its curvature in different directions, and therefore its focal length, will vary. There are obvious reasons why mirrors should err in this way more than lenses; and it is a curious fact that plates taken with mirrors *do* show some variation in scale in different directions, *e.g.* the photographs of *Eros* taken with the 30-inch mirror at Greenwich (*Monthly Notices*, lix. p. 13). If this is a *vera causa* Pritchard's rule would be essentially correct.

2. If the plate is not strictly normal to the axis of the telescope, there would be a variation in scale in different directions. But in this case Pritchard's rule would not be quite correct. If, for instance, the twist was about the line joining *ab*, then the scale value would remain unaltered for the direction *ab*, and would be altered in the direction *cd*; but the distance from *c* to the star might be increased, while that from the star to *d* was diminished, or *vice versa*.

3. If the distances on the plate in the direction *ab* were *measured on a different day* from those in the direction *cd*, then Pritchard's rule would be essentially correct. The small variations would then depend partly on the different expansions of the plate with temperature.

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Further reasons for treating the Oxford determinations of stellar parallax with scant respect are mentioned by Sir David Gill, viz. the statements of Mr. H. S. Davis in the *Annals of the New York Academy of Sciences*, vol. x. p. 160. The following are Mr. Davis's words :—

"In the formation of Table XXIII., giving the various values of the parallax of 61 *Cygni* hitherto published, I have not placed Pritchard's values in bold type because they cannot be regarded as trustworthy. They have been discarded in taking the means of the determinations for 61^1 and 61^2. I have examined the eight sets of normal equations given in the work on 61 *Cygni* (pages 17–63), and among the eight sets have not found one which is correct in every quantity. The set on page 30 first attracted my attention, and so it may be used for illustration. As given there the quantities are :—

$$\begin{array}{rlll} -\ 1''\cdot3140 = & +88\cdot0000x & -6\cdot7889d\mu & -\ 2\cdot5442\pi \\ +\ 4''\cdot3827 = & -\ \ 6\cdot7889 & +8\cdot4762 & +\ 9\cdot7965 \\ +17''\cdot1716 = & -\ \ 2\cdot5442 & +9\cdot7965 & +38\cdot7724 \end{array}$$

These should be

$$\begin{array}{rlll} -\ 1''\cdot2180 = & +88\cdot0000x & -6\cdot7889d\mu & -\ 0\cdot9281\pi \\ +\ 4''\cdot3853 = & -\ \ 6\cdot7889 & +8\cdot4783 & +\ 9\cdot7854 \\ +17''\cdot1967 = & -\ \ 0\cdot9281 & +9\cdot7854 & +38\cdot7849 \end{array}$$

The criticism applies not only to the normals for 61 *Cygni*, but for many of the other stars as well. No attempt has been made to verify all the numbers in each set of normals, nor indeed to extend the test to all sets of normals given in the book ; but it can be stated that at least one number is wrong in each of the fourteen sets given on pages 17, 23, 30, 36, 47, 52, 58, 73, 75, 87, 104, 106, 107, and 113."

These remarks of Mr. Davis (which I had not been able to find until I had written the earlier part of this paper) are quite in accordance with my own experience of the work. It certainly contains far too large a number of numerical mistakes, and I have spent too many years detecting and correcting such errors to feel any inclination to defend or condone them. If Mr. Davis insists on rejecting this work because of such errors, and of the general mistrust which their existence inevitably causes, I for one shall not attempt to question his legal right to do so. I would only remark that it is quite possible to over-estimate the importance of such errors and the effect they have on the result. They are of the nature of accidental errors, and will increase the probable error ; hence they do not give any false appearance of accuracy. And as regards the main result, it is wonderful how little this is often affected by the correction of such errors when the total number of observations is large. Those who have had experience of examining masses of observations know this well.

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The example given by Mr. Davis involves corrections of about $-0''\cdot008$ to x, $-0''\cdot007$ to $d\mu$, and barely $+0''\cdot001$ to π. If this example is really representative we may consider the parallaxes as given sensibly correct; for no one attaches much importance to $0''\cdot001$ or even to $0''\cdot01$. Parallaxes quoted by Mr. Davis with approval in the table from which he excludes Pritchard's measures range from $0''\cdot20$ to $0''\cdot52$.

Description of a Floating Photographic Zenith Telescope and some Preliminary Results obtained with it. By Bryan Cookson, M.A.

The instrument which is described in this paper was constructed with a view to undertaking a determination of the constant of aberration by Küstner's adaptation of the Talcott Method. Observations for this investigation have not yet been begun, but a description of the instrument and of some preliminary results obtained with it may be of sufficient interest to justify its publication.

In the usual form of zenith telescope the level is the essential part; in the present case the level is abolished and the verticality of the axis, about which the instrument is rotated, is assumed by floating the support of the telescope in a bath of mercury.

The correctness of this assumption is shown in the paper. The principle of floatation was employed as long ago as 1825 by Captain Kater in his floating collimators (*Phil. Trans.* 1825 and 1828), but seems to have been forgotten till Dr. Chandler adopted it in 1884 and applied it with great success to an instrument of very novel design which he called the almucantar. It was shown by Dr. Chandler that the floating part of his instrument returned after disturbance to within $\frac{1}{20}$ of a second of arc of the original position; that is, it resumed its original inclination to the horizon with a higher degree of accuracy than the bubble of a sensitive level would do. A zenith telescope, then, which depends on the principle of floatation might reasonably be expected, other things being equal, to give better results than one which depends on a level. In 1891 the Georgetown College Observatory erected a floating zenith telescope (a short description of it will be found on pp. 43 to 56 of the publications of that observatory): the results obtained with it seem to have been satisfactory. This is, so far as I know, the only floating zenith telescope which has hitherto been made. Two large almucantars, however, have been recently constructed, one by Professor Sampson at the Durham Observatory (*Monthly Notices*, vol. lx. p. 572), the other by Professor Howe at the Case Observatory, Cleveland, U.S.A. (*A.J.* vol. xxi. p. 57). Results obtained with these instruments have not as yet been published.